blood to the cells which is required." In many places the sense is seriously interfered with by faulty punctuation, and we note a rather plentiful crop of misprints, especially towards the end of the book. Such are "centre nervous system," "tircuspid," "vertebræ," (for "vertebrata"), "cauda equinæ," "straining" (for "staining"), "Weber-Feehner law," "fenestra rotundis," (several times repeated), "scala tampani," "selerotic," "viteous humour." Nor do we care for the form "oculimotor." It is to be hoped that a future edition will be more carefully revised. The author has been fortunate in securing the use of the well-known and admirable figures from Quain's "Anatomy" and Schäfer's "Essentials of Histology." They add materially to the value of the work.

The Dawn of Reason. By James Weir, jun., M.D. Pp. xiii + 234. (New York: The Macmillan Company, London: Macmillan and Co., Ltd., 1899.)

THIS book on the mental processes of animals is the fruit of much original observation, and in many cases this observation has been supplemented by experiment; but, unfortunately, all the author's results are vitiated by his uncritical and biased attitude in favour of an extreme view of the mental life of animals, and there are few of his facts which the comparative psychologist would be justified in using without ample corroboration by other observers. Instinct is regarded as the great bane of psychology, and it almost seems as if the author believed it to be a special invention of those whom he calls "creationists." He poses as an ardent evolutionist, but is so blind to the most elementary principles of the evolution of mind that when a water-louse frightens some rhizopods, he can only conclude either that the latter have eyes and ears so small that lenses of the highest power cannot make them visible, or that these creatures are the possessors of senses utterly unknown to and incapable of being appreciated by man. He makes observations on spiders which show that they are differently affected by loud and soft vibrations of an organ—observations which do not even demonstrate the existence of hearing—and concludes that these animals have attained a very high degree of æsthetic musical discrimination. He has also seen a spider "intentionally beautifying" its web with flakes of logwood, and he has watched rhizopods employing their time in "simple amusement" resembling a game of tag. Nevertheless, among these extravagances, one meets with observations which would be of distinct value and interest if one had confidence in the observer.

The Arithmetic of Chemistry. By John Waddell, B.Sc. D.Sc. Pp. viii + 133. (New York: The Macmillan Company. London: Macmillan and Co., Ltd., 1899.)

THE volume does not differ essentially from other books on chemical arithmetic. Every teacher has his own method of presenting an arithmetical problem, which he often feels impelled to share with others. The author's methods seem thoroughly sound and logical, and no exception can be taken to them. There is a good deal to be said, too, for the plan of treating the calculations on a purely experimental basis independently of theories; but it is not always advisable to hold to it too rigidly. A good illustration is offered by the following example.

The author begins by showing that the combining weight of oxygen taken as 8 is thoroughly satisfactory, not only in its relation to hydrogen (1) in water, but to carbon (6) in its two oxides. It then becomes necessary to explain that this number for oxygen does not fulfil the expectations which it first raised, and that the formula for water HO(9) must be discarded in favour of H_2O(18). "It is found that while by electrolysis of water all of the hydrogen that is in the water is set free as a gas, and $\frac{1}{9}$ of the water decomposed is hydrogen; on the other hand, when sodium acts on water, only one-half as much hydrogen is set free, that is $\frac{1}{18}$ of the weight of water acted upon." It is questionable whether this explanation would carry conviction to the beginner. A plain dogmatic statement would surely serve the purpose better, until the student had advanced to a stage when he could grasp the whole question involved. The author has collected together an excellent set of examples from a variety of sources, which should be useful to teachers in elementary classes. J. B. C.

The Flora of Cheshire. By the late Lord de Tabley (Hon. J. Byrne Leicester Warren), edited by Spencer Moore; with a Biographical Notice of the Author by Sir Mountstuart Grant Duff. Pp. cxiv + 399, with a portrait of the author and a map of the county. (London: Longmans, Green, and Co., 1899.)

THE manuscript of this "Flora," we are told, was completed a quarter of a century ago. Those who knew the sensitive, retiring disposition of the late Lord de Tabley will not be surprised that he laid it aside as not ready for press; nor will they be surprised at the excellence of what was done. There is little beyond an enumeration of the plants of the county, but made with extreme care and with conscientious acknowledgment of doubts and difficulties in dealing with critical plants.

Two classes of vegetation seem particularly to have attracted the author's notice, and both in a decidedly historical aspect. The one class is that of the alien plants, whose spread from ballast-heaps, &c., is traced; the other is the shore vegetation of a coast which has been much changed both by man and by tidal denudation. There probably exists no "Flora" of any county in Britain which approaches it in interest in either respect, unless it be that of Middlesex by Trimen and Thiselton-Dyer, published in 1869 at the time when Lord de Tabley was at work on what has just been printed.

To the matter which was put into his hands, the editor has wisely added enough to bring the work into line with our present knowledge of Cheshire botany. The biographical notice in its want of facts is a little disappointing; and the attempt to give each plant a binomial English name leads one to a curious and not altogether happy result. These, however, are small matters. I. H. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Fourier's Series.

THE statement of Fourier's theorem for the special case which has intermittently for some months past been a subject of discussion in NATURE, is as follows:—The function whose value is $\frac{1}{2}(\pi - x)$, when x lies between 0 and π, and $-\frac{1}{2}(\pi + x)$, when x lies between 0 and $-\pi$, can be expressed by the series $\sum_{k=}^{k=\infty} \frac{\sin kx}{k}$ for values of x which lie between π and $-\pi$.

The proof of the theorem, whether in this special case or in more general cases, consists in summing the series; and the result obtained in this special case is that the sum of the series is

$$\begin{array}{ll} \tfrac{1}{2}(\pi - x), & \text{when } x \text{ lies between } 0 \text{ and } \pi, \\ -\tfrac{1}{2}(\pi + x), & \text{when } x \text{ lies between } 0 \text{ and } -\pi, \\ 0 & , \text{ when } \quad = 0. \end{array}$$

Prof. Michelson has found a difficulty in this result in that, whereas the sum of any number of terms of the series is a continuous function of x, the sum of the series is a discontinuous function of x. If I have not misunderstood him, he contends that for extremely small positive values of x the sum of the series should be regarded as indeterminate and as having any value between 0 and $\frac{1}{2}\pi$, and I understand him to support this contention by the consideration that when n terms of the series are taken, so that x being extremely small nx is finite, such an indeterminateness is found.

Such a position involves a misconception of the meaning of

the "sum of an infinite series." When $u_1+u_2+ \ldots$ is the series, the terms being uniform functions of x, the sum of the series for any value of x is the limit of the sequence of numbers u_1, u_1+u_2, $u_1+u_2+u_3$, ... in each of which x has the given value; the limit of the sum of the series when $x=0$, is the result obtained by *first* summing the series for a finite value of x, and *afterwards* diminishing x without limit; the sum of the series when $x=0$ is the result obtained by *first* substituting 0 for x in the functions u_1, u_2, ... and *afterwards* forming the limit of the sequence u_1, u_1+u_2, ... In the example in question, the results thus obtained are $\frac{1}{2}\pi$ and 0 respectively. The results that can be obtained by summing the series to n terms, diminishing x indefinitely, increasing n indefinitely and keeping nx finite, generally do not coincide either with the sum for $x=0$ or with the limit of the sum for $x=0$, when these are different. Such results may, as I have pointed out in a previous letter, be useful for purposes of illustration, but they are quite beside the mark when it is a question either of the statement of Fourier's theorem or of the sum of Fourier's series.

M. Poincaré, in his letter printed in NATURE for May 18, does not assert that the sum of the series can be obtained by allowing x to approach zero and n to increase at the same time, in such a way that nx remains finite; but he states that Prof. Michelson is perfectly right in contending that the result of this process is indeterminate. So far as I am aware this contention has not been called in question in the course of the discussion.

Oxford, May 19. A. E. H. LOVE.

Bessel's Functions.

THE remarks of "C. G. K." (p. 74) concerning the defects of style which are frequently observed in the writings of scientific men, lead me to point out a grammatical error which is creeping into mathematical literature. I allude to the use of the incorrect phrase "Bessel Functions" in the place of "Bessel's Functions."

In certain cases the name of a person may be converted into an adjective by the addition of an appropriate termination, of which such words as *Elizabethan* and *Victorian* are examples; but to use the name itself (which is a noun) as an adjective, is a violation of one of the most elementary rules of grammar.

When the conversion of a proper noun into an adjective would be cumbrous or inelegant, the only correct mode of expression is to use the *genitive* case. If, therefore, we reject such an adjective as "Bessellian" on the ground of its inelegance, we must use the phrase "Bessel's Functions," that is functions of Bessel. The absurdity and incorrectness of the phrase "Bessel Functions" is at once seen by comparing it with such phrases as "*Green* Theorem," "*Chrystal* Algebra," "*Love* Elasticity."

The correct use of the genitive case is a subject upon which considerable misapprehension has existed. Thus we find in the Prayer Book the phrase "For Jesus Christ His sake," instead of "For Jesus Christ's sake." The error arose from the fact that the compilers of the Prayer Book were ignorant that the *'s* is not a conception of the pronoun *his*, but is the old Teutonic genitive which still exists in most German languages.

Fledborough Hall, Holyport, May 28. A. B. BASSET.

"The Art of Topography."

IN your issue of March 23 (No. 1534, vol. lix.) appears a review of "Recherches sur les Instruments, les Méthodes et le dessin Topographiques, par le Colonel A. Laussedat," signed by "T. H. H." The review brought to my attention several points of interest upon which I beg leave to comment.

Regarding planetable instruments, the reviewer says "that 'Russians and Americans' use very complicated instruments." Of the Russian instruments I have no knowledge, but this is certainly not true of the American.

The U.S. Geological Survey makes use of the planetable to a greater extent than any and all other organisations in America, fully two hundred of these instruments being constantly in use.

The instruments used are remarkable in simplicity and efficiency, are reasonably light, portable and accurate. The instruments are of a model designed by Mr. Willard D. Johnson, of the Survey, and are fully described on pages 79 to 89 of *Monograph* xxii. of the U.S. Geological Survey, entitled "Manual of Topographic Methods," by Mr. Henry Gannett.

This work also treats of the methods of accomplishing topographic mapping by the Geological Survey. Mr. Gannett explains the use made of the planetable, and shows that all work is controlled by points, located by triangulation or other means dependent upon numerical measurements and carefully computed. The triangulation is carried on with eight-inch theodolites reading, by micrometer microscopes, to two seconds.

The instructions to triangulators include the order that points must be selected and arranged so as to best control the area under survey, and that *three* points at least should be located on each atlas sheet of the map. Since these sheets differ in area in different parts of the country, ranging from 1/16 of a square degree to a square degree, the distance between triangulation stations necessarily varies considerably.

After the primary triangulation points are located in an area, dependence upon the planetable is absolute for the "secondary" triangulation within that area, the control, both horizontal and vertical, is carried on by use of this instrument. If the surveyor using a planetable for graphic work starts from accurately located points with check point available, he very soon discovers any "accumulation of error," in that it is impossible to make the several locations check one with another.

In regard to the use of "continuous contours" to express relief, the "Commission of 1826" seems to have drawn the remarkable conclusion that for scales less than 1 : 10,000 this system is insufficient.

The Geological Survey publishes topographic maps which vary in scale between 1 : 9600 and 1 : 250,000 (1 inch to 800 feet and 1 inch to 4 miles about, respectively), and on these maps the contour interval varies between 5 feet and 200 feet. The expression of relief is, I think, in these cases satisfactory, at least so far as giving accurate information is concerned; the artistic effect is very good also, especially when the topographic features are large and the slopes steep, cliffs appearing as broad heavy lines where differentiation of the individual contours is impossible.

About 1890, the use of mercurial barometers was abandoned by the Geological Survey, and trigonometric methods for obtaining heights were adopted. At the present time the primary heights are determined by spirit-levelling, from which elevations are carried in connection with the triangulation or by lines run with vertical angle readings and carefully measured distances. The use of the aneroid barometer is only allowed in inaccessible areas between the known elevations, and must be frequently checked. The experience of the writer in widely separated regions in the United States, in obtaining differences of elevation with the aneroid, leads him to the conclusion that, as a rule, the instrument fails to record differences as accurately when carried from a higher to a lower region as it does when the change of elevation is in the opposite direction. Also, that an aneroid which has been used in a region of elevation of given range must be given time to accommodate itself, if it be required to do good work in a region of greater or less elevation than that in which it has been used. The principle and construction of the aneroid is such that it never can be accepted as an instrument of precision except within well-defined limits, with frequent comparison with known elevations. The Survey has in use several hundred aneroid barometers, but no confidence may be had in any one of them unless frequently checked, as stated. It will be seen that the methods now in use in America agree more closely with those practised by the British Government, at least so far as the Colonial surveys are concerned, than with any other of the European surveys. R. H. C.

The Heating of the Anti-Kathode in X-Ray Work.

SINCE the beginning of X-ray work the heating of the anti-kathode has caused great difficulty, and with the introduction of the Wehnelt interrupter it is even more important that this should be prevented. In other words, we all along have had more energy from the coil than could be utilised in the Crookes' tube. Many workers like myself have tried to remedy this, and various plans have been adopted to keep the anti-kathode cool. It occurred to me that if we could get a piece of platinum, fused into the glass tube itself, to act as the anti-kathode, and placed opposite the kathode, this object might be attained. Such a tube, after many attempts, has at last been made; and although the first experiments have only been successful in making small tubes, others of a larger size are at present being attempted. The advantage of this method will easily be seen, because the heating of the piece of platinum can be prevented by placing the whole tube in a fluid cooling mixture or otherwise. These tubes are difficult to make at present, but I possess one which has retained its vacuum for some weeks.

179 Bath Street, Glasgow, May 28. J. MACINTYRE.